

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXII.

June 13, 1862.

No. 8.

Dr. LEE, President, in the Chair.

David Smith, Esq., Birmingham ; and

John Robertson, Esq., Glasgow,

were balloted for and duly elected Fellows of the Society.

On the proposed Parallax Observations of Mars in 1862.
By Prof. C. Piazzi Smyth.

In the important paper of Dr. Winnecke, of Pulkova, in the *Bulletin* of the St. Petersburg Academy, entitled, "Considerations concernant les Observations Méridiennes à faire pendant l'Opposition prochaine de *Mars*, dans le but de déterminer sa Parallaxe," that able astronomer sets forth — and, as we may assume, satisfactorily — first, the large amount of uncertainty at present existing in the received value of the Solar Parallax (probably $\frac{1}{30}$ th of the whole); secondly, the extreme desirability of improving this determination; and, thirdly, the unusually favourable opportunity which the opposition of *Mars* offers this autumn, beyond and before most of the recent oppositions, for attaining to the desired consummation. He then proceeds to select a series of stars admirably calculated by their position to be compared in declination with *Mars*, each night from August 20th to November 3d, and, finally, to announce that his method of observation will be bisection, or its equivalent, at Meridian transits with a meridian transit-circle.

Now every one knows that whatever may be done at all with a meridian transit-circle will be done by such an instrument at the Central Observatory at Pulkova, with an accuracy which will be an example to almost every country; but yet one may express surprise that in this case a *meridian*-circle should have been chosen; for the object proposed to be immediately gained is, a micrometrical difference only between *Mars* and the stars of comparison, and is therefore more suitable to a large *Equatoreal*, especially as the number of days during which the parallactic effect will be at its maximum is very small, and observations should rather be accumulated at that particular time than thinly spread over many months of smaller parallax.

Of course the intelligent author of the paper referred to has a reason for the opinion he has come to; and it appears to be, firstly, that in so high a northern latitude as Pulkova, measures taken with an *Equatoreal* "at great distances from the meridian, as recommended by Mr. Airy, would be attended with very grave inconveniences;" and, secondly, that he has not been certified that there will be any observations of any kind made at any Southern Observatory.

With regard to the first of these reasons, I cannot say anything with authority; but with regard to the second, it appears to me a duty to one of the absent Astronomers of the South referred to (*viz.* Sir Thomas Maclear, at the Cape of Good Hope), to mention, that I had the honour of receiving from him a letter, towards the end of 1860, detailing his observations of the *Mars* opposition of that year, and describing them with such extraordinary enthusiasm, and looking on them as so essentially his bounden duty, that I at least entertain no reasonable doubt but that he will attend to the opposition of 1862 as honestly and as ably. Furthermore, I should mention—though without any official authority so to do, and subject to correction—that Sir Thomas had made all the observations of 1860 with the 6-inch *Equatoreal* of the Cape Observatory, and expressed himself perfectly delighted with the superior power of that instrument for that purpose (*viz.* measuring micrometrical distances between *Mars* and certain stars near him) over the meridian instruments which he had been obliged to employ on former occasions; and he stated his belief that one night with the said *Equatoreal* was about as good as fifteen nights, I think, with the meridian instruments.

Hence there seems not only a chance amounting to all but certainty, that the *Mars* opposition of 1862 will be observed in the chief Southern Observatory, but that it will be observed with an *Equatoreal* in preference to a meridian instrument; and with this *Equatoreal* the observations will be made—according to the plan of 1860—not so much at great distances from the Meridian, as on and near the Meridian: a position which, it is hoped, will allow the great Pulkova telescope to

be similarly employed, without any of the very grave inconveniences which had been feared for it.

Here terminates, therefore, the first part of the subject; but then immediately forces itself upon our attention this case, whether, granted that the two established Observatories of the North and South fully perform their local parts, is the desired end likely to be obtained—viz. not only an accurate determination of the parallax of *Mars*, but a something so exceedingly accurate that it shall command the attention and elicit the respect of all other astronomers as well, besides those who have assisted at the observations, and to a degree which shall induce them to alter the parallax of the Sun as deduced from the transit of *Venus* in 1769?

In seeking an answer to this requirement, we are met at once by the unflattering circumstance, that oppositions of *Mars* have been observed again and again of late years, and without any such decided step being gained as would warrant an alteration of the received parallax of $8''.5776$. What, then, can be particularly said of the opposition of 1862, that should lead us to expect much better results from it than from its predecessors? This is a point which has not escaped the acuteness of Dr. Winnecke; and he accordingly mentions—besides previous observations North and South not being very comparable—that the planet itself was not on those occasions at its least terrestrial distance, which it will be very nearly at in 1862, “or only 0.4 from the Earth, the Earth’s mean distance from the Sun being 1.0.”

In referring to the *Nautical Almanac*, however, it appears that whereas the parallax of *Mars* will be $21''.2$ on Sept. 28th, 1862, it was no less than $21''.9$ on July 20th, 1860; consequently the opposition of that year was more favourable in so far by $\frac{1}{30}$, and constituted, therefore, the season when all the observations concerned should have been working for the parallactic difference, rather than in 1862. The Northern Astronomers were, however, hindered in their part of the scheme by their joining the eclipse expedition to Spain—the eclipse occurring on the 18th July, and the *Mars*’ opposition observations extending over June, July, and August. This appears to give some excellent hints; for, if many *Mars* observations were lost on that occasion, it has been allowed on the other hand by all persons, that the eclipse gained immensely by the said expedition; and, in truth, expeditions are not only most powerful means for making the most of any special astronomical phenomenon, but they have never yet had full scope given to them; although their results have been, in almost every case when they have been tried, successful. What for instance, it may be asked, would have been the transit of *Venus* in 1796, but for the numerous geographical expeditions that were fitted out to observe it from special and peculiar points of the earth’s surface? To this question

we would hardly answer, "Nothing," but rather, "A something so little accurate, that it would have yielded long since to very ordinary *Mars*' opposition observations."

If, then, the *Venus*' observations were, by astronomical expeditions, rendered so much more powerful for their useful end than they otherwise would have been, why should not the *Mars* observations receive the same auxiliary aid? The weaker astronomical phenomenon (for getting at the Solar parallax) surely needs all the more geographical magnifying of the effect, if its result is to be used in correcting the conclusions of the other. But if this magnifying be not done, then, no matter what result the present fixed Observatories arrive at, they will inevitably *not* be considered by the astronomical world to have settled the parallax of *Mars*; for they so plainly leave an opportunity to some one else, only a couple of years after, to make the same observations from a notoriously better base-line.

Had the observations been necessarily to be taken with a meridian circle, and necessarily to have occupied about three months, there might have been some reason for leaving the matter to fixed and established stations; but when they are more naturally to be made with an Equatoreal, as being small differences only, and might all be comprised most profitably within ten days or a fortnight, they come legitimately within the compass of astronomical expeditions; and, viewed in that light, the opposition of 1862 has this decided advantage over that of 1860, that it occurs at an equinoctial season of the year, when the light and heat circumstances of stations in high latitude both North and South will be very nearly similar.

In fact, at that season of the year and for the limited period demanded by equatoreal observations, there would seem to be no reason why an observer should not be sent to the North Cape, latitude $+71^{\circ}10'$, and another to the southernmost island of Cape Horn, latitude $-55^{\circ}48'$, and thus observe with an intervening difference of 127° , in place of 94° only, the angle between Pulkova and the Cape.

There is little time left now to arrange such expeditions; but we often find, that it is precisely the shortness of time given wherein to perform any work, which calls out men's energies in accomplishing it; and, in such a point of view, there may be still quite time enough. On the understanding, too, that measures on, and near, the meridian, will be made at both stations, rather than at great hour-angles, a very portable folding dome, like Capt. Jacob's, might be easily arranged to allow of limited openings in definite directions, combined with full security to the instruments from inclement weather; and if only a week, indeed only three fine nights, could be secured about the time of maximum parallax, a remarkable micro-metrical result might be obtained, and such a one as has not yet been seen in the history of Astronomy; obtained, too, not by anything so costly as what is usually called an expedition,

for the whole thing would rather partake of what the French would term "*une promenade astronomique*."

Even this time, too, might perhaps be shortened, and the result improved, by supplementing the hand-micrometer measures, with photographic pictures of *Mars* and the neighbouring stars, taken in the field of the telescope at intervals through the night on collodion plates, and submitted to screw and microscope measurement on returning to this country.

Royal Observatory, Edinburgh,
June 12, 1862.

*Extract of a Letter from M. Winnecke to the Rev. R. Main,
dated Pulkowa, 1862, May 30.*

"I had the pleasure, a few days ago, to send you a short notice, in which a common plan of action is recommended for the observations of the approaching opposition of *Mars*. You, equally with myself, will be impressed with the importance of the questions at issue; and you will, I hope, at the same time, in looking through my proposals, see that they have not been made without mature consideration of the circumstances. I venture therefore to request that you will use your authority to set on foot, as far as possible, for the approaching opposition, a system of co-operation in relation to meridian observations of declination of *Mars*.

"I add here a few short remarks on some Variable Stars, which you will probably find an opportunity to communicate for the *Monthly Notices*.

"In No. 2, vol. xxii., of the *Monthly Notices*, which I received some weeks ago, Mr. Knott draws attention to the variable star R *Vulpeculæ* = *Piazzi*, xx. 457. The minimum is, according to his estimations, nearly coincident with a predicted maximum. In *Pogson's Ephemeris* my name is given as the authority for the time of maximum. I did certainly, two years ago, communicate to Mr. Pogson elements of this star; but I was afraid that an error of transcription had crept into them. My elements, which represent seven maxima, observed in the course of three years, with reference to *Piazzi's* estimations of magnitude, in August 1803, are namely

Maximum, 1860, Nov. 6 + $138^d.6n$

(n being the number of periods from 1860, Nov. 6);

from which the maxima of 1861 fall on March 25, August 10, and December 27, and those next to be expected should occur on

1862, Sept. 30; 1863, Feb. 18, July 4, Nov. 20.